General comments

The authors investigate the role of vertical bedrock displacement on the marine ice-sheet instability. To this end, they sample various combinations of the lithospheric thickness and upper-mantle viscosity, and use an idealised setup that mimics some of the important features of Thwaites Glacier, which is likely to undergo a marine ice sheet instability in the coming centuries. By forcing the system with a very slowly evolving meltrate from the ocean, the authors assess the bifurcation point of the system. By using a forcing with significant rate, they simulate a collapse of the glacier below the bifurcation point, a situation also known as R-tipping. For some parameter combination, they observe a back and forth migration of the grounding line position across the unstable region at a constant forcing. This results in three main messages of the paper: varying the lithospheric thickness and upper-mantle viscosity can (1) shift the bifurcation point, (2) lead to R-tipping and (3) give rise to self-sustained oscillations.

The main messages conveyed by the authors are undoubtedly relevant for past and future marine ice-sheet evolution. The setup of lower complexity eases the understanding of the mechanisms at play and facilitates a clear description and visualization of the results. The text is easy to follow, the figures are nice and informative. It is obvious that the authors put great effort into the present work and I am sure that it will be a valuable contribution for the community.

However, I believe that a few important points need to be checked/modified before a possible publication.

The authors find that the lithospheric thickness, T_e, plays a significant role in shifting the B- and R-tipping point. The explanation for this is found in Fig. S2, where it is shown that the uplifted bathymetry is very different for each choice of T_e. On many occasions, the authors mention that smaller values of T_e lead to a more localized response, but it really looks like the decisive factor is that the response is emphatically larger. For instance, the maximal bedrock uplift yields nearly 1,000 m for T_e = 20 km but only about 300 m for T_e = 200 km. Besides the fact that the authors should emphasize this more and move Fig. S2 to the main text, I am very surprised by this result: other studies have shown that, although T_e affects the gradient and rate of the displacement, it only marginally affects its equilibrium magnitude (Whitehouse et al., 2012; Milne et al., 2018; Coulon et al., 2021; Swierczek-Jereczek et al., 2024). I propose a simple way to verify this in the specific comments.

However, I believe that the strong dependence on T_e cannot be solely explained by the differences in uplift and the authors do not dwell into the details of this, which is a shortcoming that needs to be addressed. I suggest the following explanation. The bathymetry of the side walls is close to 0 around the coastal sill. Thus, any significant bed uplift suppresses basal melt there and provides lateral pinning points that greatly hinder the grounding-line retreat. Therefore, the role of T_e in shifting the bifurcation point so strongly might be largely due to the choice of the bathymetry. Given the large amount of work already provided by the authors, I don’t think they should perform additional runs to investigate this. However, they should show heatmaps of the fully uplifted bathymetry (with a colour bar that changes rapidly around z=0, for various T_e, optionally in supplement, not only transects of the centreline) and critically discuss the fact that their setup is particularly sensitive to bedrock uplift because of the large pinning-point potential, right before the retrograde portion of the bedrock.

Furthermore, the update frequency of the LC model is set to 10 years, although the authors infer a characteristic timescale of the solid-Earth of 7.5 years for eta = 10^{18} Pa s. I think the simulations involving low values of eta need to be performed again with a higher update frequency, unless the authors can prove that this is not critical. Of course, the LC model is unconditionally stable since it uses an implicit time-stepping scheme, but numerical stability does not imply numerical accuracy.

The authors use the case of a rigid Earth as a baseline to quantify R-tipping (L. 15-16, L. 250-258, Eq. 2, Fig. 6) and therefore compare setups with different physics (GIA on vs. GIA off). This is not the idea of R-tipping. However, it is easy to address by comparing simulations with the same parameters (T_e, eta) but different rates, i.e. modifying Eq. 2 to:
$\Delta m_{R\text{-tip}}(eta, Te) = (m_{R\text{-tip}}(eta, Te) - m_{B\text{-tip}}(eta, Te)) / m_{B\text{-tip}}(eta, Te) * 100\%$

I am confident that the authors will be able to address these points, and I wish them the best of luck for the rest of the publication process.

Specific comments

1. The title focuses on R-tipping, but the paper also highlights how certain solid-Earth parameter choices can shift the bifurcation point or give rise to self-sustained oscillations. Therefore, I feel like the title could be more general, something like “Solid-Earth structure conditions the stability of marine-terminating glaciers”. But this is up to you.
2. Mentioning the large rate of the anthropogenic global warming compared to the past temperature record would be a nice complement for the motivation of studying R-tipping.
3. L. 60-61: As mentioned above, T_e should not affect the vertical displacement significantly in the LC model.
4. L. 69. You say that R-tipping remains largely unstudied in the MISI context, without any citation. However, in the discussion, you cite Swierczek-Jereczek et al. (2025), which has already suggested the timescale separation between ice and GIA to be responsible for R-tipping. This feels odd, especially because you therefore miss a nice motivation of your work in the introduction: to date, R-tipping of the AIS was studied, but never in a setup of lower complexity, where it is easier to identify and isolate the mechanisms. This is an simple but necessary fix to embed your work well in the state of the art.
5. Your model description is really nice and easy to follow, but I feel like the GIA part misses some important information. What is the upper-mantle viscosity? Furthermore, to impose sensible boundary conditions, the LC model requires the ice-sheet domain to be smaller than the GIA domain. What is your choice here? Especially when it comes to the y-dimension, it is not an obvious choice since you assume periodic boundary conditions in the ice-sheet model which are difficult to

consistently treat in the GIA model. From what I see in your plots, the vertical displacement is always very close to zero at x = 800 km, which makes me wonder if you chose the GIA domain to correspond exactly to the ice-sheet domain. Since I am not sure of this, the lines below are speculative.

If you chose your GIA domain to be the same as your ice domain, it would be critical for the experiments with large T_e: since the displacement spreads significantly, it might be that u_viscous << 0 at x = 800 km. However, following Bueler et al. (2007), you would impose u_viscous = 0 there (on average). This might offset your displacement and be responsible for the large changes in displacement magnitude depending on T_e, which what I mentioned in the general comments.

6. To validate the setup with respect to this concern, I would like you to perform a standalone run of the GIA model with T_e = 80 km, assuming a rectangular ice load centred in (x, y) = (0, 0), with L_x = 1200 km, L_y = 320 km, thickness = 1 km at t = 0 and remove the load instantaneously after. Please specify what is your GIA domain and upper-mantle viscosity so that I can verify that we obtain similar results.
7. L. 143: Based on what you say here, I think it would be really nice if you would highlight the plausible solid-Earth parameter combinations in Figs. 5 & 6. This would ease the interpretation.
8. L. 173: I feel like it's a pity to not show your results from branched-off simulations and step experiments in Fig 4.a and Fig. 5 (triangles and circles). This would allow the reader to see the results rather than a post-processed version of it. Please include this.
9. L. 206-208: I understand that what you describe here is just a consequence of the differences in the fully uplifted bathymetries. Based on Fig. S2, it looks like the decisive factor here is whether the fully uplifted centreline bathymetry has any emerged part. Could you confirm/discuss/stress this? This also conditions the possibility of overshoot, which you mention at l. 329, but I think not with sufficient emphasis: the decisive role of GIA here is because it modulates the nonlinearity in basal melt (= 0 if z > 0; > 0 if z<= 0).
10. L. 215: Not sure that the ice shelves are the reason for the readvance: even without buttressing, you observe a similar behaviour, as shown by Schoof (2007); Pattyn et al. (2012).
11. L. 339-340: You should mention that this solid-Earth parameter combination is unlikely.
12. In the discussion, you should mention that MISI can be stabilised by other mechanisms, e.g. upstream increase of precipitation (Sergienko, 2022). This is important, since in the present setup you prescribe constant accumulation and therefore miss this effect.
13. Really neat parallel with binge-purge mechanism and nice link to Holocene readvance!

References:

- Sergienko, 2022: No general stability conditions for marine ice-sheet grounding lines in the presence of feedbacks.
- Whitehouse et al., 2012: A new glacial isostatic adjustment model for Antarctica: calibrated and tested using observations of relative sea-level change and present-day uplift rates: A new GIA model for Antarctica.

- Milne et al., 2018: The influence of lateral Earth structure on glacial isostatic adjustment in Greenland.

Technical corrections

i. L. 1: “may be characterized by” → “will likely be subject to”
ii. L. 2-5: This sentence is long, a bit confusing and would benefit from being split into two sentences. “West Antarctica” → “West-Antarctic Ice Sheet”; “most” → “more”.
iii. L. 7-8: “basal ice-shelf melting” → “sub-shelf melting”
iv. L. 8: “solid Earth structures” → “solid-Earth structures,”
v. L. 10: “B-tipping threshold” → “bifurcation point”
vi. L. 14: “that for half of the ensemble members rate-induced tipping (R-tipping) occurs” → “that rate-induced tipping (R-tipping) occurs for half of the ensemble members”
vii. L. 16: “effective critical tipping threshold” → “effective tipping threshold”
viii. L. 27: mention a percentage of the WAIS contribution to the AIS ice loss.
ix. L. 34: “van den Akker” is missing a hyperref. This applies to many of your citations throughout the paper, e.g. “Kachuck” (l. 53), “Fürst” (l. 83), “Pattyn” (l.86). This also applies to all the references you are making to figures (e.g. l. 41) and tables (e.g. l. 113). Please check this thoroughly.
x. L. 35-36: cite Swart and Li at the end of the sentence.
xi. L. 38: “on bed” → “on a bed”
xii. L. 41-43: “One important stabilizing factor in the MISI context is the buttressing effect of ice shelves that are laterally confined or grounded on topographic highs” → “The buttressing provided by laterally confined ice shelves is an important stabilizing factor of the MISI”.
xiii. You often use the phrasing “MISI-type retreat”, as in l. 49. I would simply write “MISI” in most cases, or “MISI-driven retreat”. Also at l. 132.
xiv. L. 72: “for” → “of”
xv. L. 117: “(Cornford et al., 2020, Eq. 3 of)” → “(Eq. 3 of Cornford et al., 2020)”.
xvi. L. 127: “reaches equilibrium after several 10,000 model years” → just mention the exact number of model years.
xvii. L. 134-135: this is a good sanity check for your model setup but I am not sure if it needs to be mentioned in the paper. Up to you.
xviii. L. 202: “does only occur” → “only occurs”
xix. L. 209: “several 100 m” → “several hundreds of metres”
xx. L. 228: delete “that could counteract GL retreat”
xxi. L. 240: “solid-Earth structure,”
xxii. L. 307: “10km” → “tens of kilometers”
xxiii. Fig. 1: “GL retreat” → “GL retreat on retrograde bed”
xxiv. Fig. 3: Would read better in kyr
xxv. Fig. 9: “reverse MISI” → “MISI”. The former sounds a bit odd to me, since it’s just MISI, following Schoof (2007).
xxvi. Some references don’t include all the required information. For instance, the journal is missing for Adhikari et al. (2014) and some weird information (bandiera_abtest: a Cg_type, Group Subject_term, Subject_term_id…) is given for Adusumilli et al. (2020). Please check the whole reference section thoroughly.